in perfect condition. If not in good order, especially at the points of attachment, the current is interfered with. A sudden rupture during the operation would cause a severe and painful shock to the patient. Her involuntary movement might dislodge the uterine trocar, and occasion a wound in some adjacent part.

A contrivance for irrigating the vagina, both before and after the operation, must be provided. The fluid used is a solution of the sublimate or of phenol. This is also to be employed for washing the hands; and the speculum, sounds, and trocars, with the handles, should be immersed in it when prepared for use. The celluloid sheath of the sound or trocar especially requires cleaning after every service, and its canal must be scrubbed out with a pipe-brush in boiling water or the phenol solution. The sounds and trocars, instead of being much rubbed, are better purified by boiling water or the flame of a spirit lamp. So much for the appliances; at another time, the operations and the cases for which they are fitted will be described.

ON SOME LATER EFFECTS OF RICKETS.

By J. A. COUTTS, M.B. CANTAB., M.R.C.P.,

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IN a disease like rickets, presenting such marked and obvious effects in the osseous system, it is scarce matter for surprise that the constitutional symptoms are often, or generally, overlooked and under-estimated. Looked upon and described by the older writers, from the time of Glisson and Whistler downwards, as a purely local affection, it was close upon the middle of the present century before the general and, in my opinion, more important symptoms came to be recognised. But, even then, attention was almost entirely directed to such matters as splenic and hepatic enlargements and derangements of the pulmonary and digestive organs, and, beyond the casual recognition that rickety children were more liable to convulsions than others, but little stress was laid upon the important evidences of suffering furnished by the nervous system. As proofs of this suffering, besides convulsions, might be instanced the frequent occurrences of such complaints as hyperæsthesia, mental backwardness, tetany, laryngismus stridulus, muscular weakness, loss of cremasteric reflex, etc. When the number and frequency of these complaints is taken into consideration—and it is remembered that some of them, such as laryngismus and tetany, are seldom seen apart from it—there can remain but little doubt as to the implication of the central nervous system in rickets. Now the reason of this involvement is not far to seek. Rickets is a disease of malnutrition, produced either by improper feeding or by some cause, inherited or otherwise, which prevents the proper digestion and assimilation of food, which may be perfectly correct in quality and quantity for the time of life in question. It may seem to some that I place the digestive factor in too high a place as regards the causation of rickets, but few, I take it, will doubt the prominent part played by it in most cases of the disease. In the effects of this malnutrition almost every organ and tissue of the body may be implicated, and it would be strange if so complex and delicate a structure as the infantile brain should suffer no damage. That it does not escape this damage is evidenced by the neuroses so common amongst rachitic children, and they may be regarded as the natural outcome or expression of the cerebral malnutrition. Assuming, then, that the infantile brain suffers damage during the rachitic period from the general malnutrition, it may be asked: Is this damage only a temporary one, restricted to the rachitic period, or does it ever render itself evident by any signs in later years? The answer in most cases, no doubt, will be of the first kind, and, just as enlargements of the bony epiphysis, spleen, etc., may all disappear and leave no trace of their former presence, so any marks of cerebral disturbance may vanish, and the child grow up as intelligent and free from neuroses as one that has never suffered from rickets. That the answer should be in all cases of this favourable character is, I would contend, hardly to be expected from the nature of the case. It must be remembered that the period of the rachitic suffering is that of the greatest physiological activity and evolution of the brain, and that damage or malnutrition is more likely to be resented and prove harmful than then at any subsequent time. The amount of harm done, and its evidences, are hard to estimate and to recognise in so complicated an organism as a young child, but simpler yet strictly analogous examples are to be found if we turn from the animal to the vegetable kingdom; probably the amount of harm done to a growing plant by the withholding of nutriment or by malnutrition is inversely proportional to the age of the plant. But just as in rickets our chief guides to diagnosis are the changes produced in the bones, so, it may be, the disappearance of them may possibly conceal the origin of some of the

sequelæ of the disease. It is well known that children may be profoundly rachitic in infancy, and yet present no traces of it in the bones a few years later on.

Of the later effects of rickets, it is my intention to deal only with those that concern the nervous system. Most others, but not all, come under the province of the surgeon. Some of these neuroses are directly connected with the actual time of suffering from the disease, and are continuous into later life; others are separated from it by varying intervals of time.

This question as to the connection of neuroses in later life with a past rickets is not a new one. Dr. Gowers, in his book on *Epilepsy*, page 23, out of a large number of cases, ascribes at least 10 per cent. of them to this cause. Nearly two-thirds of his cases beginning in infancy, the conditions of origin of which could be ascertained, arose from so-called dentition-convulsions. "Few persons," he writes, "who have attended closely to the diseases of children can fail to be convinced of the truth of the opinion, long ago urged by Jenner and now widely accepted, that almost all convulsions associated with dentition are really due to the constitutional condition of retarded development which we call rickets—that is, to the irritability of the nervous system which accompanies this condition. The further details of the cases I am considering fully confirm this; for in a large number of cases there was a history of late walking, late teething, and in many of crooked limbs. But these facts do not fully represent the influence of rickets in the production of epilepsy. In many of the cases just described the course of the attacks was continuous from infancy to adult life. In others, however, there were intermissions, especially during the period of childhood. In several, for instance, the fits ceased at 7, to commence again at 10 or 11 years. Now, in twenty-seven other cases which commenced after the period of childhood was over, repeated convulsions had occurred during the first dentition, accompanied, in many, with other signs of rickets. It seems reasonable to ascribe to these convulsions of infancy a share in predisposing to the convulsions of later life, and the addition of these cases raises the number in which a causal influence may with probability be ascribed to rickets to nearly 10 per cent. of the whole."

With these words of Dr. Gowers I cordially agree. My own experience of epilepsy, founded almost entirely on cases in childhood, gives as high a percentage as 17 which had been preceded by rickets in infancy. This larger percentage of mine is possibly due to the age of the subjects with whom I have chiefly had to deal, or is accidentally connected with my much smaller number of cases. In these cases there was a history of rickets, with infantile convulsions, followed by epilepsy in later life; but it is an allowable hypothesis that a similar state of cerebral irritability or malnutrition may exist in many cases, and yet convulsions be absent owing to absence of a direct exciting cause. Then, if such a condition, when accompanied by convulsions, is responsible for epilepsy in later life, it is quite conceivable that without this accompaniment it may be the cause of epilepsy or other neuroses at times of greatest stress upon the nervous system—that is, at second dentition or puberty. It is the associated cerebral condition and not the mere convulsions, its outward and visible signs, which must be held the responsible agent for any after-neuroses. Thus, had Dr. Gowers chosen to extend his cases of epilepsy due to rickets beyond those in which convulsions were present in infancy, he might have fairly ascribed a larger percentage even than 10 to this causal influence.

I have taken epilepsy in the first place, not as being the most frequent of these possible sequelæ of past rickets, but because, on such good authority as that of Dr. Gowers, its connection with infantile convulsions allies it more immediately with its cause than any other neurosis.

Turning next to chorea, one of the most frequent and well-marked neuroses of childhood. It has been for some time past my custom to inquire in cases of chorea as to a history of rickets in earlier life, and the search has not been unproductive. Out of fifty-one cases of chorea, in which such inquiry was made, in eleven there was a fairly reasonable history to warrant an assumption of earlier rickets. This, in several of the cases, was confirmed by the permanent bony changes produced in the head, chest, or limbs. Thus, assuming my deductions as to the past history to be correct, this would give as high a percentage as 21 for cases of chorea which had been preceded by rickets. The rheumatic hypothesis to account for chorea has, I fear, been urged by its adherents too far at the present day to admit of any other possible cause receiving favour. But a past rickets as a predisposing one would not exclude rheumatism as an immediate exciting cause in many cases. Moreover, the most enthusiastic advocates of the rheumatic theory have to admit that, in many instances, some other factor besides it is necessary to explain the origin of the complaint.

Whether past rickets and chorea stand to one another in any other relation beyond an accidental one may be an open question. Of their frequent association I entertain no doubt. It is easy to test the matter by trial, and anyone doing so will, I venture to think, be convinced of the truth of this association. Dr. Donkin, on the other hand, in his able and interesting analysis of 105 cases of chorea (*Westminster Hospital Reports*, vol. 1) states "rickets was notable by the absence of its signs." But he mentions that four of his cases had convulsions when infants, a fact in itself strongly suggestive of rickets, and that in many other cases inquiry as to convulsions had not been made. The question as to the former presence of rickets is not to be judged merely from the absence of bony changes, but by the careful and special inquiry into the past history on this point, and in Dr. Donkin's cases I cannot find that such special inquiry was made. In such a number of children, most of them taken from the east of London, it would be strange indeed if some of them had not suffered from rickets as infants, and, if true, would give rise to the suggestion that freedom from rickets in infancy predisposes to chorea in later life.

Of mental backwardness I have spoken as being one of the symptoms that prove the involvement of the central nervous system in rickets. So extreme, indeed, may be this backwardness that it is difficult at times to settle whether a case is one of rickets only or of congenital mental defect; and it is interesting to mark the improvement of the mental along with that of the rachitic conditions. These are the cases more especially that have been spoken of under the description of "cerebral rickets," and were supposed to indicate hypertrophy of the white cerebral matter in the sufferers. As regards this hypertrophy I know little beyond the fact that it has the great name of Hilton Fagge as one of the vouchers for it. Most pathologists would find it hard to prove or disprove its existence in any given instance; and, in my view, the supposition of such a condition of the cerebral matter is unnecessary for the explanation of any of the nervous disorders in the disease. Although in most children the mental backwardness is limited to the rachitic age, yet in others I believe it persists throughout life. I see many patients with a past history of late teething, late walking, etc.; and the mothers complain that they are as far behind other children in learning and intelligence in later years as they were behind those of their own age as infants.

The connection of headaches with past rickets has been noticed by more than one observer besides myself. Dr. Day, on headaches in children, writes: "If the headaches go on, the head gets larger at the sides and more prominent at the forehead; and this appears all the more obvious as the child loses flesh and the face shrinks. In rickety children this headache is not uncommon, and the recovery is the more slow and tedious." With the writer I am at one as regards the frequency of headaches with the shape of head described, but I am far from agreeing with him as to the shape of the head being the consequence of, or even subsequent to, the headaches; and still further from accepting the pathological reasoning furnished by him as accountable for the complaint. From the short description given the head-shape would seem to be that which is not uncommon amongst children who have suffered from rickets, and both it and the accompanying headaches I am inclined to attribute to that disease.

Migraine in children has been described as presenting all the same characteristic features as in adults, with the addition of feverishness at time of attack. My own experience of typical cases of it in children has been too limited to admit of my including it as one of the neuroses having a possible origin in rickets.

Besides neuroses presenting such marked characteristics as those already dealt with, there are many of an ill-defined nature common in rachitic children. These are such as tooth-grinding, somnambulism, night terrors, etc. In connection with somnambulism, I have under my present care three markedly rachitic children who suffer from it: one, a child of three years old, with marked enlargements of the epiphyses and bandy legs, who has walked in her sleep almost from the time of learning to walk, three months ago.

Many others of these children, again, can only be described under the general and unsatisfactory term "neurotic;" presenting such symptoms as ill-development, wasting, nervousness, etc. Many such afford examples of the nervous hand posture, described by Dr. Francis Warner, along with over-mobility of the digital phalanges, tongue, etc.—signs, according to the last named writer, indicative of brain ill-nourishment, leading to instability either of nerve-centres or cells.

In what has gone before I have treated the subject by endeavouring to discover in children who were neurotic what traces were afforded of a past rickets. But it might be looked upon from the other point of view—namely, what evidences of neuroses are furnished by children who have been undoubtedly rachitic in infancy.

In my note-books I find records of forty-three cases in which were well-marked evidences of a past rickets, judging by the permanent changes in the osseous system; but of these forty-three cases, twelve were distinctly of a neurotic temperament—that is, as nearly as possible, 28 per cent. Of these neurotics, two were epileptics, three somnambulists, one choreic, five suffered from recurrent headaches, and one was subject to attacks, described as faints, which were judged to be *petit mal*. This would seem to confirm the view that children who have suffered heavily from rickets are as much prone to neuroses in later life as they are during the actual time of the duration of that disease.

These views as to the connection of neuroses with a past rachitic condition are only put forward as possibilities, and not as being capable of rigid demonstration by aid of statistics. Indeed, from the nature of the case statistics can furnish but unreliable evidence as to their truth. This will be manifest when the variety of the symptoms in rickets, their frequent escape from detection, and the paucity of traces left behind by them, are taken into account; and the whole matter must be viewed in a general light. But if it may be assumed that in rickets the brain, at its most active period of evolution and development, suffers from the general malnutrition, and that this malnutrition leads to instability of the nervous centres, then, I would contend, the question passes the limit of the possible and enters that of the probable. This instability of the nervous system may be permanent, although revealing itself to us by no known methods of research until some greater strain than ordinary is made upon it, as at second dentition or puberty, for instance. That the intervening times are free from manifestations of neuroses need not militate against the view of such a condition of nervous system. We have ample illustrations of permanency of such a cerebral condition, with intermittency or infrequency of its manifestations, in such diseases as epilepsy and migraine. The same arguments apply to the objection that no pathological changes are found *post-mortem* in the brains of those dying with rickets. In but few cases of epilepsy, I take it, are any pathological changes found *post-mortem* in the brain sufficient to account for it, and yet few would doubt the cerebral origin of the disease. Of course I am aware that it might be urged against these views as to the connection of neuroses with a past rickets, that the same condition of malnutrition might be present in older children, owing to the same want of food of proper quality or quantity, just as in the rachitic period. In answer, I would state that, in the case of these children in the East of London, the question of ill-feeding rests not so much on the absence or deficiency of food that may be in itself perfectly nutritious, but on the fact that it is unsuitable for the time of life. The food that is administered to, and pressed upon, the unfortunate infant, is probably perfectly suitable for children of the age with which I am dealing. In but few of my hospital patients of more advanced age have I any reason to suspect the possibility of starvation. I would not deny, however, that it may be an accessory factor in some cases. Another objection, and one harder to meet, is that children, as seen at the East London Hospital, who may be entirely free from any neurotic suspicion, would present nearly the same percentage of rickets amongst them as those with any neuroses that I have dealt with. This, from the difficulty of ascertaining and determining the normal percentage, can never, probably, be satisfactorily answered. If one judged from infants merely, the percentage of rachitic ones would rise to 40 or 50, or even higher. But it must be remembered that these infants were probably brought to the hospital on account of suffering from rickets, and any percentage drawn between them and the rest of the children of their own age would be fallacious as determining the amount of rickets in the whole community. This clearly does not apply in the case of older children who are past the rachitic age. Taking this fact into consideration, and remembering the high mortality amongst rachitic children, I do not think the percentage of those who have suffered from rickets in infancy would amount to as high as ten, even, amongst my older hospital patients. But, as I said before, the question does not admit of solution by statistics of this sort. A partial answer to the objection is that where rickets is most prevalent there neuroses are most frequently met with.

If these views—suggestions rather—as to the remote sequelæ of a past rickets, should be accepted as probable, then my short paper would be a plea for the further consideration of the importance to be attached to the complaint, its prevention, and treatment. Few diseases are so readily preventable by proper attention to infantile diet and hygiene, or so amenable to proper treatment when incurred. Even those cases which are not preventable can be profoundly modified in their severity by the means taken to arrest its advent. It must be remembered that the mortality returns are no indication of the number of deaths attributable to rickets. Like syphilis, seldom

or never fatal *per se*, it is frequently so owing to its complications, which get the credit of causing death, and the original cause escapes mention in the death-certificate. Apart from the liability to death from the complications associated with the disease, rickety children are much less able than others to withstand the numerous and manifold ills that the infantile flesh is heir to. In spite of this, too often the comforting assurance is given to mothers "that the child is only rickety, and will get all right in course of time, and no further anxiety or trouble need be incurred." That this masterly inactivity and trust in time is often disastrous in its results is only too well known to both physicians and surgeons connected with children's hospitals. Moreover, that 10 per cent. of adult epilepsies, on high authority, have their origin in rickets, should call also for a more guarded prognosis in many cases of the disease, more especially when it is allied with convulsions. The all-prevalence of rickets, and its association with the English name, still remains a reproach to us as medical practitioners.

ON THE UNSATISFACTORY RESULTS OF UNILATERAL REMOVAL OF THE UTERINE APPENDAGES.¹

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In previous publications I have had repeated occasions to point out that, under the term "removal of the uterine appendages," in order to keep a clear logical notion of the actual details of the proceedings, it is necessary to make such subdivisions as will indicate the purpose for which the operation is undertaken.

One writer has maintained that operations should not be classified on any other grounds than their mere anatomical relations, and that the intentions and purposes of the operation should not enter into the element of classification at all. This, of course, is a conclusion which will not bear investigation for a moment, for we are at once met by the difficulty that it would be impossible to recognise any distinction between the operation for the induction of premature labour and the crime of abortion-mongery, unless we had distinctly in mind the intentions and purposes of the operation. Not only so, but in the case of an amputation at the lower third of the thigh for a smashed knee-joint, there are precisely the same anatomical relations as when the operation is done at the same point for chronic inflammatory disease, yet it has long been the practice to separate in statistical tables primary amputations from those of a secondary character. We have therefore many points to take into consideration besides mere anatomical details in the classification of operations. This is strikingly the case in the operations for removal of the uterine appendages, where we have a clear and precise division at once rendered necessary by the fact that, in certain groups of cases, it is absolutely essential to remove the appendages on both sides.

Thus if we operate for the arrest of the hæmorrhage of a myoma, or for the purpose of reducing its size, it would be perfectly futile to remove the appendages on one side only, unless those of the other side were already absent. So if we desire to prevent impregnation in such rare cases of deformity as I think may justify the removal of the uterine appendages, in order that risk of life may not be incurred, both sides of the uterus must be rendered sterile.

On the other hand, actuated by the sound principle that no organ should be removed which is not diseased, in all the cases of the various forms of chronic inflammatory mischief in the uterine appendages which have come under my care, I have not in a single instance removed the second set of appendages when they have been ascertained to be healthy. At first sight it would appear as if there is no need for interfering with the second side when only one side is diseased, whether the disease be characterised by occlusion and distension of the tube, or by dense adhesions which render the functions of the organ impossible, and give rise to such intolerable suffering as to make life a burden. But first-sight conclusions and *a priori* arguments are often found, upon larger experience and more careful research, to be fallacious; and I greatly fear that the conclusions which I have arrived at on this point, and upon which I have up to the present acted, are not likely to bear the test of careful investigation. I have been made painfully familiar with the frequency with which operations of this kind have proved absolutely useless for the pur-

poses of the operation, and where the disease has recurred on the other side, and demanded a second surgical interference.

The time has therefore come when I should put on record the evidence in my possession, and leave for the judgment of my professional brethren a question which is certainly novel and startling. The question is whether it would not be better to advise the complete removal of the uterine appendages in any case where an operation is demanded by the presence of serious disease on one side only.

As the first contribution to the solution of this important problem, I have submitted the histories of all the cases of unilateral removal of the uterine appendages on account of chronic inflammatory disease operated upon by myself up to December 9th, 1884, at which time I completed my first series of one thousand cases of abdominal section. The reasons for this selection are, I think, sufficient, the strongest of them being that the time which has elapsed from then till now gives me a fair period, though by no means a complete one, at the conclusion of which to give the after-histories of the cases. It will be seen from what I have to say of some of these cases that the complete results will probably not be evident for two or three years to come, and by that time the evidence will be stronger, I believe, in the direction, which is clearly indicated at the present moment, that if we have to remove one set of appendages for chronic inflammatory disease, it will be far better to remove them both.

The cases are twenty-seven in number; and as the operation proved fatal in one of the cases the inquiry is limited to twenty-six. This group may be subdivided as follows:

	Cases.
Abscess of ovary	1
Chronic ovaritis with adhesion	2
Hæmatosalpinx	4
Hydrosalpinx	4
Pyosalpinx	15

These twenty-six cases form, curiously enough, as nearly as possible, one-fourth of all the cases that I operated upon, during the period of one thousand cases, for chronic inflammatory disease of the uterine appendages. But the relations which this group of twenty-six cases have to the general relations of the total cases of this kind operated upon indicate very remarkable conclusions. Thus pyosalpinx is unilateral relatively to hydrosalpinx as about seven is to four; hydrosalpinx is unilateral relatively to hæmatosalpinx as about four is to one; and hydrosalpinx is relatively frequent to chronic ovaritis with adhesion as eight is to one. Without being exactly cognisant of what these relations might prove to be on careful examination, I was abundantly aware that the commonest cases of pelvic suffering amongst women, matting of the pelvic contents, or glueing of the ovaries and tubes to all the other organs, with the occlusion of the tube and its distension either by serum or pus, were almost uniformly bilateral.

I was also perfectly aware of the fact that hydrosalpinx was almost uniformly symmetrical, but I have been profoundly struck with the curious fact that we may find a large pyosalpinx densely adherent, with its corresponding ovary, to other organs on one side, and a perfectly healthy set of appendages on the other, and it was the frequent recurrence of second operations in this class of cases which struck me so forcibly as to lead me into this special research, the more so as it was also in this group that I met with cases of death from neglect of second operations.

We now know that pyosalpinx has a lethal significance far more serious and extensive than any of us could have dreamt of in the beginning of this kind of practice, some seven or eight years ago. On the contrary, whilst I cannot say that I am free from suspicion that hydrosalpinx is occasionally fatal, I cannot be very well brought to believe that its risks are great, and I do not think that we could imagine chronic ovaritis with adhesion having a fatal result. But the curious thing is that, over the whole group of these diseases, the amount of suffering is not in proportion to the risk of life which is run, but is directly the reverse.

I have over and over again removed large rotten Fallopian tubes, distended with eight or ten ounces of pus, when there has been hardly any pain at all, and where the symptoms have been almost entirely confined to mere general constitutional disturbance.

Only a few weeks ago I removed from the wife of a medical practitioner in this town a huge unilateral pyosalpinx just on the point of bursting, which undoubtedly had not existed more than twelve or fourteen days. With equal certainty this pyosalpinx would have burst and killed the patient within a week, and yet she had no great pelvic pain from the beginning of her illness to the end of it. I had the utmost difficulty in persuading her husband to permit me to perform the necessary operation; but when it came to be performed in his own presence, nothing could exceed his expressions of gratitude for

¹ Abstract of paper read before the Birmingham and Midland Counties Branch.